*Note on Mr. A. B. Basset's Paper, "Theory of the Action of Magnetism on Light."**

By Joseph Larmor.

I have recently been favored by Mr. Basset with a copy of his paper; my apology for offering a few remarks on a criticism of a discussion of the subject by myself, which forms a main topic in it, is that I hope to be able to state some definite information which may be worth recording in the American Journal of Mathematics.

What Mr. Basset chiefly deprecates is a method that I have indicated for modifying the theory of magnetic action given originally by Maxwell on the hypothesis of an interacting continuous dynamical system, so as to make it fit in with the ordinary doctrine of reflexion and refraction of light. That theory, as it was applied to the problem of reflexion by Fitz Gerald, does not involve a sufficient number of variables to satisfy the conditions and lead to definite results, such as experiment reveals, independent of accidental conditions at the interface. The theory being dynamical, this implies that some restricting condition in the problem has been overlooked: the variables ξ, η, ζ are in fact not independent, so that the dynamical variational equation must be subject to the condition that they satisfy, which involves the introduction of a Lagrangian parameter λ. This Mr. Basset apparently appreciates, and he admits that this new variable obviates the difficulty above mentioned: but he proceeds (loc. cit. §6) to compare this homogeneous theory with Maxwell's equations for an electric field in which there is no rotational influence, and he cannot make out that the one is a sufficiently simple modification of the other. This I think is what the criticism means: but his discussion is based on purely arbitrary interpretations, which

* American Journal of Mathematics, vol. XIX, p. 60.

lead him to the conclusion that the theory is also open to the same objections as was his own. The question is, however, does the theory form a scheme consistent with *itself?* Mr. Basset had already, as he now admits, tried to make the Maxwell equations work, without success: why then should he reject *a priori* without trial the modification of these equations which does work, on the ground that it is not sufficiently slight—for as to logical difficulty there is none? As a matter of fact, the modification is excessively slight in the sense that λ would be a very minute quantity that could not be detected in any other way than by experiments on reflexion from magnetized metals. It is complained that there is no definition of the electric force as involving this minute effect: the answer is that there was no necessity to define it, wherein lies a merit of the method. If Mr. Basset so desires, he can run down the analysis of the Maxwellian scheme from the Principle of Least Action, when the rotational property is non-existent, and notice how the quantity usually defined as electric force there comes in: he will thus see that what would correspond to components of electric force, in a rotational medium, are the expressions on the left-hand of his equations (23). He complains that no physical meaning is assigned to λ: this I suppose indicates a change in his dynamical views, as a few years ago he inserted in the first volume of his Treatise on Hydrodynamics a deduction of the equations of fluid motion from the Principle of Least Action, in which an exactly similar parameter arises by introducing the condition of continuity of flow into the equation of Action. The obvious physical interpretation of the parameter is there the fluid pressure: so here it would represent a hydrostatic pressure, on the hypothesis of an incompressible aether, but if we refrain in Maxwell's manner from particularizing our ideas of the aether we must also refrain from interpreting the parameter.

As to Mr. Basset's modification of his former theory in §10, I agree with him that this is a simpler and in that respect more satisfactory scheme than the one we have just been discussing, because it keeps closer to the spirit of Maxwell's ordinary electrodynamics,—in fact for reasons of molecular theory which I have been intending for some time to set forth in another connexion. But why should Mr. Basset claim it as his special property? In the very paper, a section of which (§§8–11) he is criticising (British Association Report, 1893), he will find it indicated as an alternative method of obtaining a logical scheme of magnetic reflexion. The whole discussion (§§13–20) is of course too long to transcribe: but I quote §20, and take the opportunity to restore the accidental

omission of a coefficient, and to give a more conclusive, in fact decisive, reply to the difficulty that is there pointed out at the end.*

"There are thus two ways in which the magnetic field may affect the phenomena of light-propagation. The imposed magnetization is an independent kinetic system of a vortical character which is linked on to the vibrational system which transmits the light waves; the kinetic reaction between the two systems will add on new terms to the electric force: these terms are naturally continuous so long as the medium is continuous, but owing to their foreign origin they need not be continuous at an interface where the magnetized medium suddenly changes. At such an interface the other part of the electric force, which is derived from the vibrating system itself, has been assumed to be continuous in the ordinary manner, viz. its tangential components continuous; the total induction through the interface must of course always maintain continuity. This seems to be the type of theory developed by Maxwell in his hypothesis of molecular vortices ("Treatise," §822), and the conditions to which it leads have been applied to magnetic reflexion by the majority of writers on the subject, including Basset, Drude, J. J. Thomson. But against this procedure there stands the pure assumption as regards discontinuity of electric force at an interface. The correct boundary conditions would be derived from the modification of Fitz Gerald's procedure, which has been explained above.

The other point of view is the purely formal one contemplated by Lord Kelvin and Maxwell in their discussions of possible rotational coefficients introduced into the properties of the medium by magnetization. The magnetization is supposed to slightly alter the structure of the medium which conveys the light-vibrations, but not to exert a direct dynamical effect on these vibrations.

It would appear from the analysis of Drude, and more particularly of J. J. Thomson,† that there is some ground for assuming the correctness of the equations to which the former method leads; and those equations may be

*The section which Mr. Basset quotes as my theory is entitled "Dynamical Theories based on the Form of the Energy-function": the following section, of which the above quotation forms the end, is entitled "Recent Electrical Theories." In a subsequent memoir, which Mr. Basset characterizes, where the aim was to get as much aether theory as possible out of a single energy function of a continuous medium, the theory involving λ was the only one that fitted in; but in the appendix to that memoir, and in a second part since published, it was shown that this foundation must be broadened by separate consideration of aether and of matter; and I hope shortly to show in a continuation of the same subject that the hypothesis of molecular constitution therein laid down leads naturally to magnetic rotation of the type of the other theory above described.

†J. J. Thomson, "Recent Advances in Electricity and Magnetism," 1893, §412.

expressed in the terms of the second method somewhat as follows.* The electric current is in a dielectric the rate of change of the electric displacement, which is of an elastic character; in a conducting medium part of the current is due to the continual damping of electric displacement in frictional modes: it may thus fairly be argued that the fundamental relation is primarily not between current and electric force, but between current and displacement, while the current is indirectly expressed in terms of electric force through the elastic relation between displacement and force. The equations would then run as follows, (ξ, η, ζ) being the electric displacement:

$$(u, v, w) = \left(\frac{d}{dt} + \frac{4\pi\sigma}{K}\right)(\xi, \eta, \zeta)$$

where

$$\xi = P - b_3 Q + b_2 R,$$
$$\eta = Q - b_1 R + b_3 P,$$
$$\zeta = R - b_2 P + b_1 Q.$$

This would make the relation between electric displacement and electric force of a rotational character, owing to the magnetization. If the medium were not magnetized, Lord Kelvin's argument might be employed for the negation of such a rotational character on the ground that a sphere rotating in an electric field would generate a perpetual motion; but, as it is, the rotation in the magnetic field would generate other electric forces. The frictional breaking down of displacement, viz. conduction, is known to assume a slightly rotational character, as manifested in the Hall effect."

On this I now make the following remarks. In the equations connecting the electric displacement (ξ, η, ζ) with the electric force (P, Q, R) the coefficient $K/4\pi$ has been accidentally dropped out from the first terms on the right-hand side. On referring back to §15, it will be seen that the coefficients b_1, b_2, b_3 involve d/dt, and so are each of the form $\alpha d/dt + \beta$.† Now there is no possibility of this relation admitting perpetual motions, as above suggested, if for transparent media the term β vanishes in each coefficient: this I had in that connexion omitted to notice, although it was in fact stated in §16 in a quotation

* "When this transformation is made, Drude's boundary conditions become simply the ordinary ones which express that the tangential components of the electric force and the magnetic force are continuous in crossing the interface; the difficulty as to discontinuity in the tangential electric force does not now occur," *loc. cit.* §18.

† It is to be borne in mind that the general equations are stated throughout for the general case of conducting media.

from Willard Gibbs' general discussion of the subject. Moreover a brief consideration of the case of non-metallic media shows independently (loc. cit. §5) that β must vanish, as otherwise the law of dispersion would be entirely different from Biot's law of the inverse square of the wave-length.

Of the two alternative valid magneto-optic schemes thus indicated, the latter leads to the same analysis for the problem of reflexion as Mr. Basset's in Phil. Trans., 1891, and is in fact his present modified scheme. This analysis should also at bottom form a case of the more recent general discussions of Goldhammer and of Drude in their memoirs in Wiedemann's Annalen, 1893, in which the theory is put to the test by means of the actual observations on magnetic reflexion from metals. In this latter connexion I am bound to state that the theory in its valid form as regards boundary conditions was really first formulated by Goldhammer, though without attending to the question of the possibility of perpetual motions (loc. cit. §14); but the unusual character of his notation and analysis had obscured the fact from my notice.*

Although we may form a preference for one or the other of the two schemes from general considerations, the only real test between them is the criterion of agreement with experimental data, which are now voluminous and exact, thanks chiefly to the Dutch physicists. This test is not an easy matter, because the phenomenon is measurable only in reflexion from the magnetic metals, so that the complications of metallic reflexion enter into the analysis. But when the data for such precise verification exist, the mere deduction of formulæ for the simple case of reflexion by transparent media is a minor consideration. I am permitted by Mr. J. G. Leathem, Fellow of St. John's College, Cambridge,† to state that he has worked out a detailed comparison of the theory involving the parameter λ with the experimental data of Kundt, Sissingh, and Zeeman, and has found that the agreement is at best very doubtful; also that it is probably better with the second type of theory, with which also in its valid form he became acquainted through my British Association report, and of which his test is not yet complete.‡ As above mentioned, the discussions of Goldhammer and

*I find however that I had introduced all these considerations in a discussion of magneto-optic theory published in the Proceedings of the London Mathematical Society, April, 1893, where Goldhammer's scheme is stated explicitly just as Mr. Basset now gives it, and the negation of perpetual motions is also verified.

† Mr. Leathem has pointed out to me that by an oversight I have incidentally stated that the value of λ must be continuous across an interface: this is of course not true, so that λ is expressible as the potential of a surface layer *plus* double sheet.

[‡ The complete discussion has fully verified this anticipation. May 1.]

Drude relate to the second type of theory: the latter author takes the rotational constant for metals to be a complex quantity of special form related to the complex refractive index, while the former takes it to be an unrestricted complex quantity. The result of their examination of each other's work in Wiedemann's Annalen seems to be an admission that the experimental determinations employed for purposes of verification were not sufficiently exact to decide between these alternatives. I anticipate that we shall soon have further and perhaps more definite information.

One other point may be noticed, to prevent the misconception that might arise from silence. Mr. Basset remarks (§13) on what he considers to be the fallacy of placing the Principle of Least Action at the foundation of physical dynamics. The question involved is one of deep philosophical interest, and has been a prominent subject of discussion ever since the times of Fermat and Maupertuis. As recent sources of information, reference may be made to a series of papers on the physical meaning and application of the Principle of Least Action, by von Helmholtz (of which the most important is a Memoir in Crelle, vol. 100) republished in the third volume of his Wissenschaftliche Abhandlungen; also to the introduction to Kirchhoff's "Vorlesungen über Mathematische Physik." But possibly Mr. Basset's objection is not serious: for I observe that he has immediately afterwards closely followed my example, by formulating his modified scheme in terms of the Principle of Least Action on the exact lines of Fitz-Gerald and myself. This is, in my view, an important improvement in method, for the founding of the analysis on the Action of Principle has the merit of (to a great extent) avoiding the necessity of separate investigation as to whether perpetual motions are evaded; that it could be simply made I had not noticed, or I should certainly have mentioned the fact in my British Association report of 1893. Although the theory is thus expressed in terms of a magneto-optic energy function, it of course does not follow that it thereby falls under Maxwell's idea of an interaction between two linked dynamical systems: the origin of these energy terms is to be sought in the constitution of the individual molecules, for they involve the continued product of the imposed magnetic field, the electric polarization, and its time rate of change, whereas a direct energy term belonging to the medium treated as continuous would involve the product of only two physical factors.

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